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AXIOMATIC MODELS OF RISK AND DECISION: AN EXPOSITORY TREATMENT

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I. Introduction

When Sam Kirkpatrick invited me to present a paper at this panel, I accepted somewhat hesitantly. Sam expressed an interest in a paper which would expound on axiomatic models of decision making under risk (DMUR) and perhaps compare them to models arising from the social psychological tradition. Although I have been teaching and applying axiomatic models of risky decision making for some years, I must confess that prior to Sam's call I had seldom crossed the disciplinary border of social psychology. But, on the assumption that this paper at the least would force me to expand my intellectual horizons, I agreed to write it.

During Christmas vacation I resolutely marched off to the library armed with a stack of citations from an earlier Kirkpatrick paper. The reading sessions which ensued proved very enlightening. I noted first that the social psychological approach is primarily inductive. Through repeated experimentation a fact is seemingly established; then researchers hypothesize about the mechanism which could produce such a fact. In contrast, the axiomatic approach, of course, is deductive. The models are intended as primarily normative or prescriptive, not behavioral. One sets down axioms which purport to embody principles of consistent decision-making, then ascertains which decision rules satisfy the axioms. Second, I noted that in social psychological studies

risk is defined rather simply -- even casually -- and in relative terms. Of two alternative courses of action, the one with lower probability of success is the more risky. In axiomatic models risk is defined in different ways, a fact which the body of the paper will make clear. Third, in social-psychological studies I noticed that research attention seems to focus more on the differences between risky decisions made by individuals and those made by groups, rather than on the processes of individual DMUR. The latter receives attention only indirectly when researchers attempt to explain the differences between individual and group decisions. In contrast, axiomatic models of DMUR place the individual at center stage. Numerous models focus on collective decision making, but these all build on some specific model of individual decision making.

What of the findings of social-psychological studies of DMUR? The phenomenon of the risky shift I found both intriguing and puzzling -- intriguing because it seemed to be a solid, replicable fact, puzzling because I could not understand the nature of the fact. I personally find it almost impossible to answer the questions on the Choice Dilemma Questionnaire (CDQ). Consider the following:

> Mr. H., a college senior with considerable musical talent has the choice of going to medical school to become a physician, or of entering a conservatory of music to become a pianist--a career where success would not be as assured as it might be in the medical profession. Imagine that you are Mr. H. (or advising Mr. H.). Listed below are several probabilities or odds that your musical career would be a success. Please check the lowest probability that you would consider acceptable for him to go to the conservatory.
>
> Place a check here if you think Mr. H. should not go to the conservatory no matter what the probabilities of success.
>
> The chances are 9 in 10 that his musical career would be a success.

> The chances are 7 in 10 that his musical career would be a success.
>
> The chances are 5 in 10 that his musical career would be a success.
>
> The chances are 3 in 10 that his musical career would be a success.
>
> The chances are 1 in 10 that his musical career would be a success.

Now obviously a crucial bit of information is lacking: namely, what are the relative values Mr. H. attaches to being a musician, a doctor or a failure? If I am advising Mr. H., surely I need to know whether he barely prefers being a pianist to being a doctor, or whether he would sacrifice both legs and an eye to play at Carnegie Hall. Likewise, if Mr. H. failed at concert pianism, would he find happiness as a truck-driver, or would he hang himself with piano wire? Anyone familiar with axiomatic theories of risky decision making (and hopefully most others too) would not think of advising Mr. H. lacking such information. But in the risky-shift experiments subjects typically are told only that they should presume the risky alternative more attractive than the certain one. To answer the CDQ they clearly must make more precise assumptions, and this seems to me a likely source of the change in decisions between the individual and group contexts: when individuals enter the group, their individual assumptions are modified during the group interaction and information exchange. I confess, however, that I could think of no reason why the modifications invariably produced "riskier" group decisions. So, I plunged on.

I read about various explanations for the risky shift: diffusion of responsibility (Wallach, Kogan and Bem, 1962), risk as a value (Brown, 1965), information exchange (Teger and Pruitt, 1967), and others. I admired the ingenuity evident in numerous experimental

designs. I read with interest Cartwright's (1971) doubts about the factual nature of the risky shift. And finally, I came full circle. In the more recent social psychological studies of DMUR researchers have focused on the nature of individual decision processes. And to my surprise, they have applied an axiomatic model of DMUR (subjective expected utility) to explain individual decision making and account for the risky shift (Burnstein, et al. 1971; Vinokur, 1971). How ironic! After setting sail on the waters of social psychology I had landed on the familiar shores of axiomatic theory.

This little intellectual odyssey at least gave me a clear rationale for writing this paper. To know axiomatic models of DMUR is not necessarily to love them. Even should current and future social psychological research conclude that such models meet with reasonable success in the laboratory, I would advocate caution in giving them a central place in our storehouse of theoretical ideas. This paper contains some reasons for my hesitancy. In the sections which follow I will identify some of the behaviorally questionable axioms which underly the principal models of DMUR, point out some decision contexts in which these models fail, comment briefly on some newer (not necessarily better) models recently emerged from mathematical psychology, and finally, address the problem of collective or group decision-making.

II. Established Models of DMUR

Many years ago Knight (1921) proposed a trichotomous categorization of decision making contexts. According to Knight, one decides in the realm of certainty if every available action, a_i, leads to a unique consequence, x_i, (the x_i may be vectors). One decides in the realm of risk if every action, a_i, leads to a consequence, x_i, with known probability, $p(x_i/a_i)$. One decides in the realm of uncertainty if actions, a_i, lead to consequences, x_i, but the probabilities $p\,(x_i/a_i)$ are unknown or not even meaningful.*

To consider this classification more closely, I will introduce a common representation of a decision problem (Figure 1).

acts \ States	θ_1	θ_2	. . .	θ_n
a_1	x_{11}	x_{12}	. . .	x_{1n}
a_2	x_{21}	x_{22}	. . .	x_{2n}
$\vdots$	$\vdots$	$\vdots$		$\vdots$
a_m	x_{m1}	x_{m2}	. . .	x_{mn}

Figure 1

To interpret Figure 1, an act, a_i, results in a consequence, x_{ij}, depending on the "state of nature," θ_j, which obtains. For example, the set, θ, might be θ_1: a Christian God exists; θ_2: no Christian God exists; the set of acts, a, might be a_1: live a good Christian life; a_2: eat, drink and be merry; filling in the consequences is left to the imagination of the reader.

In light of Figure 1, we can say that <u>certainty</u> exists if one knows the unique θ_j which holds. <u>Risk</u> exists if one knows the probability distribution over the θ_j. <u>Uncertainty</u> exists if this probability

* As we shall see many decision theorists no longer differentiate between the contexts of risk and uncertainty.

distribution is unknown or unknowable. (What is the probability that a Christian God exists?).

In principle, DMUC presents no problem. If one can assume that the decision maker has a preference ordering over the set of consequences, then one would expect him to choose the act which leads to the most highly valued consequence. And despite the simplicity of the certainty context, by posing some additional questions and defining some additional concepts economists have constructed an interesting body of theory about individual DMUC (e.g. Green, 1971).

The best known axiomatic model of DMUR is von Neumann's and Morgenstern's (N-M) (1947) expected utility theory. To paraphrase their principal result:

EU: Given that a decision maker satisfies a particular axiom set (to be discussed below) there exist numbers $U(x_{ij})$ such that if he chooses the risky act, a_i, for which $\Sigma_j p_j U(x_{ij})$ is at a maximum, he chooses in accord with his preference for risky options.

The N-M axioms enable us to find numbers $U(x_{ij})$ (utilities or subjective values), such that an ordering of available acts by their expected utilities represents the individual's preference ordering over risky alternatives. But what are the axioms which justify this neat trick? I will illustrate them by describing a simple experiment I have performed in various classes.

I choose a subject and inquire whether he can rank order a set of potential Presidential candidates, say, Kennedy (K), Ford (F), Reagan (R), and Wallace (W). Let us assume that the student ranks the candidates K, F, W, R. This is the first axiom-- that decision makers rank consequences (both certain and risky) in a complete, transitive fashion.

Next, I give the subject a series of hypothetical choices between getting a lottery over his best and worst candidates and getting one of his middling candidates for certain, i.e. "Would you prefer F for sure or the lottery, (.5 K, .5 R)?" By varying the odds in the lottery I eventually locate a point of indifference, let us say

$$F \sim (.5\ K,\ .5\ R) \equiv \overline{F}$$

$$W \sim (.3\ K,\ .7\ R) \equiv \overline{W}$$

where $\sim$ signifies indifference.

One of the axioms (Archimedian) of EU theory states that we always can find numbers (.5 in lottery $\overline{F}$, and .3 in lottery $\overline{W}$), which render the lotteries $\overline{F}$, $\overline{W}$ indifferent to the certain consequences F, W. These numbers are the "utilities" of the theory.

At this point I ask the student to choose between two compound lotteries, say

$$L_1 = [.2\ K,\ .3\ F,\ .3\ W,\ .2\ R]$$

$$L_2 = [.4\ K,\ .1\ F,\ .1\ W,\ .4\ R]$$

while I predict his choice. In this case I would predict L_2. Between finding the lottery equivalents, $\overline{F}$, $\overline{W}$ and the choice between L_1 and L_2, three more axioms come into play.

The first of these axioms states that in every lottery in which the consequences F and W appear, we can substitute the lottery equivalents, $\overline{F}$ and $\overline{W}$, and not affect preferences over the lotteries, i.e.

$$L_1 = [.2\ K,\ .3\ F,\ .3\ W,\ .2\ R] \sim L_1' = [.2\ K,\ .3\ \overline{F},\ .3\ \overline{W},\ .2\ R]$$

and similarly for L_2. Another axiom then states that decision makers operate on lotteries according to the usual probability calculus. To wit,

$$L_1' = [.2\ K,\ .3\ (.5\ K,\ .5\ R),\ .3\ (.3\ K,\ .7\ R),\ .2\ R]$$

$$\sim [.2\ K,\ .15\ K,\ .15\ R,\ .09\ K,\ .21\ R,\ .2\ R]$$

$$\sim [.44\ K,\ .56\ R] = L_1''$$

Similarly,

$$L_2 = [.4\ K,\ .1\ F,\ .1\ W,\ .4\ R] \sim L_2' = [.4\ K,\ .1\ \overline{F},\ .1\ \overline{W},\ .4\ R]$$

$$\sim L_2'' = [.48\ K,\ .52\ R]$$

The final axiom, monotonicity, asserts that the lottery $[pK,\ (1-p)R]$ is preferred to the lottery $[p'K,\ (1-p')R]$ iff $p > p'$. In our case L_2 is preferred to L_1 given that $.48 > .44$.

In sum, by employing the N-M expected utility axioms we have determined for our hypothetical subject that U(K) = 1, U(F) = .5, U(W) = = .3, U(R) = 0.* By substituting these values into our original lotteries L_1, L_2 we have found that the expected utility of L_1 is .44 and the expected utility of L_2 is .48, and given that the subject satisfies the axioms, a choice of L_2 accurately reflects his underlying preference.

I have performed this simple experiment between six and ten times over the last few years and have not yet failed in predicting the subject's choice between the two compound lotteries, L_1, L_2, even

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*Because preference intensity is not comparable across individuals, we arbitrarily choose the unit and zero of our scale by setting U(K) = 1, U(R) = 0.

when, as in the preceding example, the expected utilities were within .1. Reason for optimism? Perhaps, but on the negative side I have also caught students consistently in a classic paradox created by Allais (Raiffa, 1968).

Consider the two compound gambles which follow.

(1)

(2)

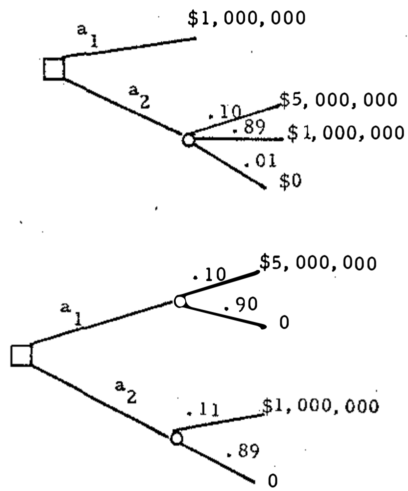

Figure 2 (Allais)

In gamble 1, choice of a_1 leads to a million dollars with certainty, whereas choice of a_2 leads to a million dollars with probability .89, five million with probability .10 and nothing with probability .01. Gamble 2 has an analogus interpretation. Now, it is not uncommon for students to choose a_1 in both gamble 1 and gamble 2. Their reasoning runs like this: "In gamble 1 I'll take the million dollars with certainty--

I'd feel terrible if I greedily tried for $5,000,000 and got nothing. In gamble 2 I'm likely not to win anything so I'll go for the $5,000,000 even though the odds are slightly worse than going for the $1,000,000." The kicker is that this pair of choices (a_1, a_1) is inconsistent for expected utility maximizers.* For, choice of a_1 in gamble 1 implies that

$$U(1M) > .10\ U(5M) + .89\ U(1M)$$

$$\text{or} \quad .11\ U(1M) > .10\ U(5M),$$

while choice of a_1 in gamble 2 implies

$$.10\ U(5M) > .11\ U(1M),$$

and these implications are obviously contradictory.

When decision theorists run experiments like the above, they sometimes remove the malefactors to a separate room, verbally work them over, then emerge to report that the offending subjects have seen the error of their ways and wish to change their choices (MacCrimmon, 1968). I can report, however, that some subjects simply prefer to be inconsistent according to the expected utility model rather than change their choices from (a_1, a_1) to something else. The problem seems to be that receiving 0 in the context of gamble 1 is different from receiving 0 in the context of gamble 2. But this in turn raises doubts about the substitutability axiom of the EU model.**

In sum, I have no doubt that the EU model of DMUR can achieve fairly good predictive accuracy in some situations. But I am equally

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*as is (a_2, a_2).

**in a slightly different context one can show that the Allais paradox casts doubt on Savage's "Sure Thing Principle." See below, pp. 13-14.

certain that one can formulate not-unreasonable examples in which significant numbers of decision makers behave in a fashion sufficiently different from the underlying axioms that the model will not predict their choices.

The N-M expected utility model is a model of DMUR. The model takes probabilities as given. But what of the more difficult case of decision making under uncertainty (DMUU)? How does one decide in the absence of theoretically derived or empirically estimated probabilities of the states of nature? In 1954 Savage proposed the classic subjective expected utility (SEU) model. The principal result is:

SEU: Given that a decision maker satisfies a particular set of axioms, there exist numbers s_j (subjective probabilities), and $U(x_{ij})$ (utilities) such that if he chooses the act, a_i, for which $\sum_j s_j U(x_{ij})$ is at a maximum, he chooses in accord with his own preferences.

Once the notion of subjective probability is accepted, the distinction between DMUR and DMUU need no longer be maintained. In principle, no probability is unknown or unknowable if probability means degree of belief rather than something more "objective." One's beliefs about the likelihoods of events may very well be based on available objective evidence such as past relative frequency data, but even lacking such data one can still have beliefs. The SEU model currently is the most widely accepted model of DMUU (or DMUR).

The Savage axiom set is more extensive and more difficult to exposit than the N-M axiom set. Thus I will concentrate on one critical axiom that has received a good deal of attention.

Consider the following decision problem:

	θ_1	θ_2	θ_3
a_1	x	y	z
a_2	y	x	z

Figure 3

Assume the decision maker prefers x to y. Then, in the Savage system, if he chooses a_1 in Figure 1, we infer that he regards θ_1 as at least as probable as θ_2. (If not, he would have chosen a_2). Next consider Figure 4 which is assumed identical to Figure 3 except for the consequences under θ_3:

	θ_1	θ_2	θ_3
a_1'	x	y	w
a_2'	y	x	w

Figure 4

One might expect that if the decision maker chooses a_1 in Figure 3, he would also choose a_1' in Figure 4. After all, although the consequences of acts under θ_3 differ in the decision problems, the portion of the decision problems on which acts differ is identical in the two decisions.

This is the essence of one of Savage's critical postulates--the well-know <u>sure thing principle</u>. The postulate demands that in situations such as those illustrated in Figures 3 and 4 the only admissible choices are (a_1, a_1') or (a_2, a_2')

To consider the acceptability of the sure thing principle let us consider an example offered by Ellsberg (1961).[*] An urn contains 90 balls, 30 of which are red the other 60 of which are black or yellow in unknown proportion. Consider the choices offered in Figure 5.

	30	60	
	θ_1 (Red)	θ_2 (Black)	θ_3 (Yellow)
a_1	\$100	\$ 0	\$ 0
a_2	\$ 0	\$100	\$ 0

Figure 5 (Ellsberg)

If the decision maker chooses a_1, he receives \$100 if a red ball is drawn, \$0 otherwise. If he chooses a_2, he receives \$100 if black is drawn, \$0 otherwise. Thirty of the 90 balls are known to be red, but anywhere between 0 and 60 balls may be black. Many subjects chose a_1.

Next consider the choices offered in Figure 6.

[*] The reader may note that the Allais and Ellsberg paradoxes are formally identical.

	30	60	
	θ_1 (Red)	θ_2 (Black)	θ_3 (Yellow)
a_3	\$100	0	\$100
a_4	0	\$100	\$100

Figure 6 (Ellsberg)

Figure 6 differs from Figure 5 only in that <u>both</u> acts now yield \$100 rather than \$0 if θ_3 obtains. According to the sure thing principle choice of a_1 in Figure 5 implies choice of a_3 in Figure 6. But students don't always see it that way. Many prefer the 60/90 chance of \$100 given by a_4 to the anywhere between 30/90 and 90/90 chance given by a_3. But choice of a_1 in Figure 5 implies

$$s(\theta_1) > s(\theta_2)_3$$

whereas choice of a_4 in Figure 6 implies

$$s(\theta_1) + s(\theta_3) < s(\theta_2) + s(\theta_3)$$

and clearly we can't have it both ways.[*] Nevertheless, intelligent subjects sometimes remain unconvinced by such demonstrations and persist in their choice of (a_1, a_4) Savage to the contrary notwithstanding.[**]

[*] The technically inclined may note that this pattern of beliefs violates the condition of finite additivity in that seemingly $s(R) > s(B)$ but $s(RUY) < s(BUY)$.

[**] In a highly interesting recent article Slovic and Tversky (1975) report that not only do subjects regularly violate the Savage Sure-Thing Principle, but that additionally Ellsberg-Allais arguments are more efficacious in inducing behavioral change than Savage arguments.

Clearly, no model of individual decision making will always predict perfectly. Individuals make mistakes; highly unusual decisions may call forth new modes of decision-making. But on the other hand if a non-trivial proportion of decision makers consistently and wilfully behaves contrary to the predictions of a given model, then at the least one may suspect that the model is missing something. Such suspicions have led some researchers to search for an alternative to the SEU model of DMUU. The next section of this paper offers a brief introduction to this newer research.

III. Newer Models of DMUR

In the EU and SEU models of DMUR valuations of consequences and likelihoods of events constitute the basis of decision. "Risk" simply describes a decision context in which these models are (presumably) applicable. In recent years, however, a number of mathematical psychologists have focused more directly on the concept of risk. They have proposed models in which valuations of consequences, likelihoods of events, and attitudes toward risk constitute the basis of decision.

For example, Coombs proposes "Portfolio Theory" (PT) as an alternative to EU and SEU theory. For Coombs a decision over uncertain prospects is ". . . a compromise between maximizing expected value and optimizing the level of risk. The nature of risk itself is undefined in Portfolio Theory . . ." (1974, p. 4). The second statement in the quotation might seem strange, but in his work Coombs makes assumptions about the concept of risk which go part way towards defining it.

Consider two gambles:

$$L_A = [px, (1 - p)y]$$

$$L_C = [qx, (1 - q)y] \qquad (0 \leq p, q \leq 1;\ p \neq q)$$

Next, form the compound gamble:

$$L_B = [rL_A, (1 - r)L_C] \qquad (0 < r < 1)$$

Now, according to the EU or SEU models, the utility of the compound gamble, L_B, must be "between" the utilities of the simple gambles of which it is composed. That is, of the six possible strong preference orderings over L_A, L_B and L_C, utility maximizers can have only the two "monotone" orderings, $L_A > L_B > L_C$, or $L_C > L_B > L_A$.

For Coombs, however, there exists something called the "risk" of a gamble. He makes two critical assumptions: (1) decision makers have preference functions over risk, functions which are single-peaked when expected value is constant; (2) given two gambles with the same expected value, a compound gamble formed from these two has a level of risk "between" them. Consider Figure 7:

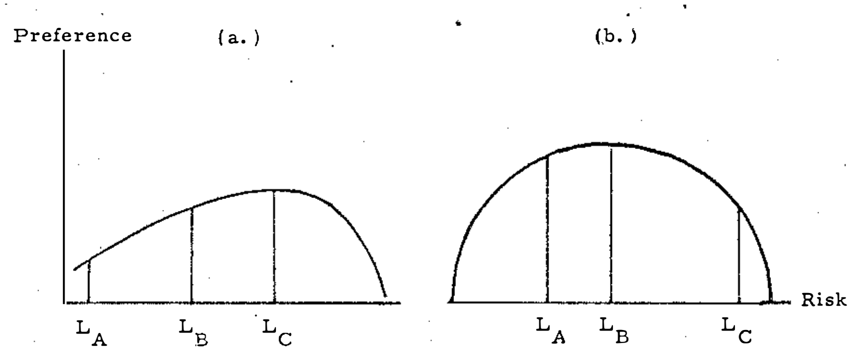

Figure 7

Assume that the three lotteries pictured have equal expected values but differ in risk (as yet undefined). In Figure 7a all of the lotteries fall to the left of the decision maker's optimum level of risk. Thus, under the assumptions of PT, he will rank the lotteries $L_C > L_B > L_A$. But in Figure 7b the simple gambles L_A and L_C "bracket" the optimal level of risk, so the compound gamble L_B is preferred to <u>both</u> of them. In general, under PT one could observe not only the monotone rankings

$L_A > L_B > L_C$, and $L_C > L_B > L_A$, but also the "folded" rankings $L_B > L_A > L_C$ and $L_B > L_C > L_A$. The remaining two orderings, $L_C > L_A > L_B$, and $L_A > L_C > L_B$ are admissible under neither theory; Coombs considers these orderings errors (and similarly for intransitivities).

The preceding arguments were tested by Coombs in two experiments. The patterns of subjects' choices were as follows:

	Exp. 1	Exp. 2
Orderings Consistent with PT and EU	54%	86%
Orderings Consistent with PT but not EU	27	9
Orderings Consistent with neither model	19	5

Table 1

Naturally enough, the more inclusive PT can account for a greater proportion of orderings than the more exclusive EU theory.

At least two questions arise from Coombs' experiments. First, what happens if the gambles differ in expected value? If L_A had an expected value of \$10, L_C, \$0, and the mixture L_B, \$5, how many folded orderings would one find? Perhaps (probably) expected utilities are virtually equal in Coombs' experiments, thereby providing latitude for a normally unimportant variable to operate.

More importantly, recall that the experiments are based on assumptions about an undefined concept. Coombs postulates single-peaked preferences for risk. Why not allow single-caved preferences as well (Figure 8)?

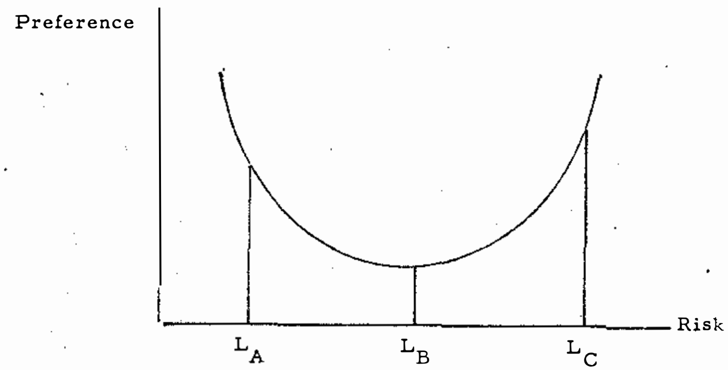

Figure 8 .

This figure would describe the risk preferences of someone who preferred big risks or none at all to moderate risks. Such preferences could generate many of the orderings Coombs considers errors and thus account for nearly 100% of the observed data. So long as we are dealing with an undefined, unobserved variable, what are reasonable assumptions to make about it?

As if to address this query Pollatsek and Tversky (1970) have provided an axiomatization of risk which is logically independent of a theory of risk preference or DMUR. Beginning with a few innocent appearing axioms they establish the following result (1970, pp. 542-543):

Risk. There exists a real valued function, R, defined over a set of uncertain prospects, L_1, L_2, L_3 . . . such that (1) L_i is perceived as riskier than L_j iff $R(L_i) > R(L_j)$; (2) the risk of the convolution of L_i and L_j equals $R(L_i) + R(L_j)$; (3) if R' is another function satisfying (1) and (2), then R' differs from R only by a positive scale constant.

With some additional axioms Pollatsek and Tversky prove the following more specific result:

Risk. L_i is perceived as riskier than L_j iff $R(L_i) > R(L_j)$, where $R(L_i)$ equals a linear combination of the variance and expectation of L_i.

Pollatsek and Tversky go on to show that if a preference ordering is a function of regular risk it can not be represented by an NM utility function. Thus, there is a clear theoretical distinction between established models of DMUR and the newer formulations of mathematical psychologists.

My intent in this section is simply to convey an impression of the provocative theories mathematical psychologists currently are developing. I think it is too early to judge the usefulness of such work for political scientists engaged in research involving a DMUR aspect, but it would behoove us to keep abreast of future developments.

IV. Axiomatic Models of Collective Decision Making

The literature in this area is far too extensive to survey here.* Instead, I will simply make some general observations about this literature which appear to be relevant to social-psychological studies of group decision making.

Axiomatic models of collective decision making have a primarily negative thrust. They focus on the problems and pathologies which attend group choice. Most conclusions take the form, "Given that a collective choice procedure satisfies axioms $A_1, A_2, \ldots, A_n$, it will not in general satisfy a (given) collective rationality condition."

*See Shepsle (1974) and Plott (1972) for reviews.

To illustrate, take the case of three decision makers each of whom has evaluated four alternatives, a, b, c, d. Our subjects have the following strong preference orderings:

1	2	3
a	d	c
b	a	d
c	b	a
d	c	b

Suppose our three subjects are to make a group choice among a, b, c, d. What if they decide to proceed by the method of majority rule? Well, they have problems. A majority (1 and 2) prefer a to b, and b to c. Another majority (1 and 3) prefers c to d. And still another majority (2 and 3) prefers d to a. Thus, the group's choice is cyclical: a > b > c > d > a and so on. The ultimate outcome is purely an artifact of the order of voting, i.e. the agenda.

Is the preceding example merely a pathological special case? Not at all. We can insure that majority rule will not produce such cycles only by placing severe a priori constraints on the preference orderings held by individuals (Arrow, 1963; Black, 1958; Kramer, 1973; Plott, 1968). Virtually every decision procedure falls subject to some pathology. As another example, what if our three man group agrees to use the method of marks rather than majority rule? The former is the familiar procedure of having individuals assign ranks to the alternatives under consideration, then selecting the alternative with the highest sum of ranks. Applied to our example the method of marks would produce:

a	b	c	d
3	2	1	0
2	1	0	3
1	0	3	2
6	3	4	5

Thus, a would be the group choice. But, what if just prior to the balloting someone told the group that the relatively low-valued b no longer was available? Well, in this case the method of marks would produce:

a	c	d
2	1	0
1	0	2
0	2	1
3	3	3

And the group would now express indifference among a, c, d. The method of marks is highly sensitive to the definition of the feasible set of alternatives, i.e. the agenda.

The preceding examples employ certain alternatives, a, b, c, d. Does anything change when risk or uncertainty is introduced? Several theoretical works (Shepsle, 1970; Zeckhauser, 1969; Fishburn, 1972) provide grounds for answering this question in the negative. Admitting risky alternatives -- in the form of lotteries over social states -- into the domain of the social choice function essentially makes a bad situation worse. Assuming that individual decision makers satisfy the EU model or even weaker conditions it can be shown that expanding the domain of the social choice function to include lotteries can upset an equilibrium where one exists in the certainty context (presumably quite rare), but can not create an equilibrium from among the newly introduced lottery alternatives. Thus, the introduction of

uncertainty into the collective choice problem only reinforces the points raised by our examples.

Axiomatic social choice theory does provide somewhat more than a background of negative results. For example, in addition to suggesting what axioms (i.e. ethical and procedural requirements) a decision procedure <u>should</u> satisfy (then proving logical incompatibility), some theorists seek to characterize existing institutions (e.g. majority rule) by specifying what axioms they <u>do</u> satisfy. Nevertheless, <u>if</u> one takes social choice theory seriously, one emerges with a rather pessimistic outlook for the empirical study of group decision making. Group choice appears to be very different from individual choice, not just a generalization of it. Group choice frequently will not appear "rational" to an external observer. Groups will be subject to manipulation in ways that individuals will not. Ultimately, even alternatives regarded as highly undesirable by most group members can logically emerge as the group choice (Fishburn, 1974).

<u>Should</u> one take social choice theory seriously? In the end I would answer both yes and no, for the kind of group choice addressed by social choice theory is a special kind in which individual preferences are presumed to be fixed. Individual preferences somehow get aggregated into a group choice, but individuals leave the arena of group decision making holding the same preference with which they enter. I have the contrasting impression that social psychologists entertain the opposite presumption -- that the process of group decision making alters individual beliefs and preferences.* I don't profess to understand the mechanisms which operate to produce agreement or "consensus" in groups, but I have obviously felt such mechanisms operate.

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*Considerable evidence for this presumption appears in the risky shift studies. See, for example Wallach, Kogan and Bem, 1962; Vinokur, 1971).

In sum, axiomatic models of collective decision are most relevant where individuals have clear preferences which are unlikely to change as a result of group interaction. Such models are less relevant where closely knit "teams" strive to make a decision. If mechanisms are at work which produce unanimity, the negative results of social choice theory typically lose their force.* Thus, I think the wisest course is to exercise extreme analytic caution in moving from the individual to the group. In some cases group choice will be analogous to individual choice. In other cases it may be exceedingly difficult for the observer to account for group choice. And we should always be careful that we _have_ the group choice as our data. Institutions (real world or experimental) generally will produce an outcome; whether such an outcome is a group choice in any meaningful sense is another matter. That observation is the invaluable contribution of axiomatic models of collective decision making.

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* The social choice theorist probably would respond to this comment with the observation that preferences for basic alternatives are not changing so much as additional considerations (desire to cooperate, deference, friendship, etc.) are being added. Fine. In this case I could simply re-phrase the argument along the following lines: the heterogeneity of preferences which underlies the negative results of social choice theory is not so empirically probable as is assumed once intangible personal factors are considered.

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